THE OWENS COLLEGE. 24

DEPARTMENT OF MEDICINE.

ADDRESS

DELIVERED AT THE OPENING OF THE SESSION
1887-8.

BY

SIR JAMES PAGET,

BART., LL.D., D.C.L., F.R.S.

PRICE SIXPENCE.

MANCHESTER: J. E. CORNISH.
1887.

THE OWENS COLLEGE.

DEPARTMENT OF MEDICINE.

ADDRESS

DELIVERED AT THE OPENING OF THE SESSION
1887-8.

BY

SIR JAMES PAGET,
BART., LL.D., D.C.L., F.R.S.

MANCHESTER: J. E. CORNISH.
1887.

ADDRESS.

I wish to speak on the utility of scientific work in the practice of medicine and surgery; not in the sense of using, for the purposes of practice, all the medical scientific knowledge that has been acquired, but in that of using, after a scientific manner, all the opportunities which practice affords for the acquirement of more knowledge. In our calling, careful practice and scientific study should be inseparable; they always may be so; and I want to urge that everyone of you should be, to the full measure of his ability, a scientific student both now and all his life-long. I have seen enough to make me sure that they who, whether they know it or not, do their work after a scientific manner are the best practitioners and contribute most to the general increase of knowledge. Although, therefore, as I have said, my chief object may be to urge the use of science for gaining fresh knowledge by the study of what may be observed in practice, yet I would have it always understood that whoever will thus work will be all the more useful to his patients, whatever kind of practice he may be engaged in.

I shall not attempt to define "science" or "scientific." Like many of the most useful words in our language, they do not admit of any exact brief definition. What I mean by them may appear as I go on. Many of you are beginning, others have lately passed through, what are especially called your scientific studies, or the scientific part of your curriculum. I could wish that they were not so called, for it seems to imply that the studies following them are not scientific; and to some minds it implies that the studies which are called scientific are not useful—both of which are serious errors. For, to any of you who so choose, clinical study, whether in your pupilage or in your practice, whether in hospitals or in private, may be made as scientific, in any fair meaning of the word, as either chemistry or physiology; and to those of you who so choose the studies of the first two years of the curriculum may be made very useful in after-life.

Whether these first years' studies are the best possible, or too many or too few, I shall not now discuss. I have spent too many hours on this question in other places to be willing to spend minutes on it in this. You must take the scheme of education which senates and councils have devised for you and do your best, and you may be sure that, when you have done your best, you will be far more fit for what is next to come than you are now. For, in all these first years' subjects, things are to be learned which, though they may be erroneously called "the foundations" of those to be learned later, are yet of great importance for the understanding of what is to follow. You may learn in them methods of study and the use of instruments for observation which are of the highest utility; and, if you will work earnestly at them, these studies will supply a very good training for that scientific method of study which ought to be associated with all the work of your after-life. It is, I believe, by this early cultivation of a scientific mind, even more than by the facts which they may teach, that these earlier subjects of medical study may achieve their greatest good. The teaching and continued study of these subjects will be the work of a few, and a constantly decreasing proportion of these few will be engaged in practice; but this makes it the more necessary that the scientific mind and method, the love of knowledge and of the means of gaining it, should abide in all those who take to the life which is called practical, and which may be scientific and is best when it is so.

Now, the first, and certainly in student-life the only safe, means for becoming scientific in our profession is the training of the mind in the power and habit of accurately observing facts. Medicine and surgery are eminently a science of observation; deductions from facts are always unsafe; I believe that they have done far more harm than good; and, for the most part, when sufficient facts have been collected and arranged, the general conclusions that may justly be drawn from them are nearly manifest. The main thing for progress and for self-improvement is accurate observation. Some seem to think it easy to observe accurately—they cannot doubt, as they say, the evidence of their senses. There are few greater fallacies. In scientific studies the evidence of the senses needs as much cross-examination as any evidence given in a criminal trial. Self cross-examination it may be, but it must be steady and severe. For by accurate observation we must mean not the mere exercise of the senses, not the mere seeing, or hearing, or touching of a thing, with some levity of thinking about it—we must not mean even the keenest use of the eye cultivated in microscopic work, or of the ear hearing sounds that to the uneducated sense would be inaudible, or the use of the finger with the most refined detective touch. All these higher powers of the senses you must acquire by careful study and practice, and you must learn to exercise them with all the attention with which a strong will can direct and watch them; but even all this, difficult as it is, is only a part of scientific observation. This must include, besides, an habitual constant watchfulness, the taking notice of all the conditions in which objects or events are found; their concurrence, their sequences, their seeming mutual relations, all their variations. To do this, and to do it again and again, and with constant care, whether it be in things occurring naturally or in experiments—to do this accurately and always is really very difficult. A few seem to have the power naturally; there are some born naturalists, some born physicists; you have had some here; but in nearly all men, and, you may safely believe, in yourselves, the power to observe accurately needs careful self-training, self-suspicion, and self-discipline.

If accurate observation were easy, how could we explain the oversights and the errors of which the evidences abound in the whole history of medicine, and which, I think, every one could find in a retrospect of his own work? Look at the history of discoveries. As a general rule, every discovery of things that can be observed tells of previous oversight or want of observation. Some discoveries, indeed, have followed quickly on the invention of improved means of observation, or on the advancement of knowledge clearing away some previous obscurities. These, it may fairly be said, could not have been made much sooner; but of far more it may certainly be said that the discoveries in each generation are evidences of the oversights, the defective or erroneous observations, of men of previous times—even of the best of them. Take some leading instances. All the facts which enabled Jenner to discover vaccination were within the range of his predecessors; they saw and heard of them, but they did not observe them; they all overlooked them, except the one farmer who vaccinated his children, but could not persuade others to follow his example. All the sounds on which Laennec established his system of auscultation were within the hearing of previous generations and of his elder contemporaries; many had heard them, but they did not listen as he did, and they did not observe the conditions in which the different sounds occurred—how each coincided with some diseased process or change of structure in the heart or lungs. All the facts by which Hunter was guided to his operation for aneurysm might have been obvious to other surgeons of his own and lately previous times. They did see them; they did not carefully observe them. I need not multiply examples. Look at any discovery of this or last year, and think whether it might not have been made in the previous year or some years ago; and when you find that it might have been and was not, though men of excellent ability were within reach of it, then you may feel that accurate and complete observation is a really difficult thing, and that for the fulfilment of your duty, whether to science or to your patients, you must cultivate the power of searching and observing with all your might.

I have been more than usually impressed with these thoughts after lately looking through many of my old case-books, and finding in them evidences of more oversights and errors of observation than I had imagined myself guilty of; and though I might console myself by the examples of the distinguished men who did not discover vaccination or the use of auscultation and did not improve the treatment of aneurysm, yet it made me think that, as an act of penitence, I ought to try to make amends by warning some who are beginning such work as I have so failed in. For instance, among my records are those of nearly all the post-mortem examinations which I made nearly fifty years ago, when I was demonstrator of morbid anatomy at St. Bartholomew's Hospital. They are full and tedious; but the evidences of oversight and of errors in observation are many. I find, for example, complete and full records of the examinations of typhoid ulcers of the intestines; and they are regarded as illustrations of the typhus fever then supposed to be prevailing. Many others were thinking the same, many had thought so years and years before; but about the same time Sir William Jenner not only saw such ulcers, but accurately observed them, and he observed all the conditions with which they alone coincided, and he proved that they were characteristic of a fever essentially different from typhus. In the same records I find long descriptions of the morbid changes in different forms of venous and capillary embolism, but I wholly overlooked and missed their meaning till it was discovered by Virchow and my own pupil Kirkes, and by others more observant than myself. I saw all these things; they accurately observed them; they studied the associated facts; and embolism, which had been destroying healths and lives for ages in secret, was at last discovered.

Do not let me seem to suggest that I deserve credit for having seen these and other like things as if for discoveries for which others have been honoured. Far from this, these are evidences of that very oversight which I want to persuade you to avoid. It is a shame rather than an honour to have seen a thing and not observed it; to have been within reach of a scientific truth and not have grasped it.

But, it may be said, these are examples of discoveries made in large fields of observation, and needing long and careful research among many cases; these are not within reach in ordinary practice. Look, then, at some simpler instances. Not a year passes but someone describes a disease of which we had no previous record—a spondylitis deformans, or some new form of myxœdema, an osteitis deformans, or some new manifestation of tubercle or cancer; it matters not what—a novelty. And the first case is thought unique, strange, and, by some, not much worth thinking of; but soon similar cases are found here and there, and the new disease seems hardly rare. I will not say that no new diseases ever appear. I have long maintained that they do: but they do not come upon us on a sudden, and we may be certain, whenever a case of new disease is observed, it is not the first that has occurred or the only one existing; similar cases have been overlooked, they have been within sight and have not been observed. And without doubt there are similar examples of oversight among us still. For example, a distinguished pathologist in Paris has lately described a disease which he names "acromegalie," with singular enlargement of the bones of the face and hands and feet. I cannot suppose that this disease is limited to France; it must, I think, be among us as well; but, so far as I know, no Englishman has yet observed it. Well, I blame no one unless it be myself; I want only to show by facts like these that it is very difficult to observe accurately, and that if you are to do your whole duty, either to your patients or to science, you must now and always study and practice the right methods of scientific observation.

I wish that the arguments for this were only in the oversights, the mere defects of observation. Unhappily the errors illustrated in all the history of medicine, and in much that is still in progress, are very numerous too. Facts, when not quite overlooked, are too often seen and recorded erroneously. I would not speak of such errors without a confession of having contributed to them, or without the fair motive of wishing to urge the best means for avoiding the like of them. For remember that many of them were made, or are being made, by men as honest as we are or ought to be, and as fairly in the pursuit of knowledge.

It would need some volumes to relate all the sources of error in scientific observation. I will mention only one, for I think it is the most frequent, and I should like you to be always watching against it. It is the habit that we have of inserting something of our own, something of our beliefs, of our expectations, nay, even of our wishes, into that which we think or say that we observe. We expect facts or events to agree with what we believe that we know, and we make light of the differences and exaggerate the likenesses; we take no thought of what we call accidental exceptions; we think them unmeaning—as if anything in nature could be without meaning—and we do not half observe them. In philosophy we can separate things from thoughts, the object from the subject; but in ourselves and in ordinary life they are mingled in every act of consciousness and reflection; and sometimes it is only by careful self-analysis and by habit studiously gained that we can separate them and observe simply and accurately.

There is a very common proverb that "seeing is believing," and many, as if trusting it, say, "I must believe what I see." It is often unwise to do so; for the sight, without the aid or control of other senses, is often fallacious. But there are many in every walk of life who, when they say "I believe what I see," might just as fairly say, "I see what I believe"; and these, though they are usually in the wrong, are usually the most positive in their assertions. They believe what they wish, and then they see what they believe; and then they become unable either to see or to believe anything contrary to their wishes—anything contrary to what they call their clear convictions. Well, as a man may see an unexpected likeness of himself in a caricature, so may we all see a defect of our own exaggerated in people such as these. We are all apt to see what we expect to see; to think that the knowledge already gained is so sure that that which is next to come must exactly agree with it. Our safeguard must be in careful scientific study, in the habit of observing without prejudice and with self-distrust, always remembering that everything brought within the range of human knowledge is brought within the much wider range of human error.

And here let me add that, however much the mechanism and other external helps for observation have of late been improved, our mental powers have not increased in the same degree; we are in ourselves as likely to observe erroneously as were our predecessors; in this sense we are not wiser than our fathers. It may even be doubted whether the improvements in the means of observing be more than in proportion to the much larger field and the more minute facts within reach of which we have been brought. It is comparatively easy now to see and do many things which used to be difficult or impossible ; but it is not, therefore, easier to see and do new things. The higher we rise on the hill of knowledge the steeper and the more difficult does the ascent become. And we are not yet near the summit : it reaches up to Heaven.

I might speak of many means of helping ourselves in the cultivation of the power of accurately observing, such as reading—which is, perhaps, of all the most important,—and of drawing, collecting specimens, and many more; but I will speak of only one, which is far too often neglected. You will find it, I think, essential to scientific accuracy that you should have the habit of recording very carefully your observations and all the chief facts connected with them. No one should wholly trust his memory in such things ; it may lose the facts, or, worse still, may gradually alter them : for, as belief and expectation and other mental states can affect the impressions which we derive from things even when they are present before us, so and much more can they affect those which we try to reproduce in memory. The self is here alone in all its fallibility.

I have heard a very truth-loving person say that if he had ever told the same story ten times he always became doubtful whether he was telling it rightly. You may do well to remember this when you are telling cases. You know the game in which a story is handed on through ten or more persons, and then there appears very little likeness between that told by the first and that told by the tenth. None of them have wilfully changed the facts, but everyone has been a little inaccurate, and the inaccuracies have accumulated. There is some measure of the same difficulty of exact representation of facts by each person's own memory, and we cannot too carefully guard against it. Inaccuracy, it has often been said, has done more harm than falsehood has; and this is certainly true in our own sciences.

Now there can be no better safeguard against these mischiefs than the habit of making studious and careful records of the things that you observe. It should be a constant habit of student-life. It is good to make notes of lectures, but better far to write down descriptions of at least some of your own dissections, and of post-mortem examinations, and of cases; and, let me earnestly add, these records should be made, whenever and as completely as they can be, while you have before you that which you are to describe, so that you may write down the very facts that you are observing, and may revise them whenever you have a chance in the presence of the same or similar facts. *Ad naturam delineavit* used to be a common inscription on portraits and other such works of art. I wish that *ad naturam scripsit* could more often be justly put upon our scientific records. We ought to be able to have it written on the covers of our note-books, and on all our descriptions of pathological changes; and our records should be full, like finished *ad naturam* portraits made by one who has again and again looked at that which he is painting, not like mere pen-and-ink sketches made after a glance or two and then swiftly put in.

I hope I shall not make your future studies seem utterly hateful to you by thus speaking of them as if they were only full of pitfalls and occasions of mischievous error. Really there are none that you may not avoid; but you cannot be too cautious; so I shall continue my advice, for most of us need to be still more careful when in our studies we pass, as we always do, from observing to thinking, and to what we believe to be reasoning. John Hunter, it is reported, used to say to those who would draw conclusions rather than search for facts, "Don't think; try." Happily for science he often did not observe the rule; he was one of the very few who could really sometimes think a truth far beyond that which could be observed; but we are not all like him, and for all of us the saying is an excellent one, "Don't think; try." Do not suppose that you can safely think what must follow from what you know; try whether it is so. Do not think that you can safely explain what you observe by any hypothesis or theory; and that having thus explained it, there is no need of studying it further. Collect new facts, and "try" your theory by them; and if the theory and the facts do not agree, stick to the facts; they will accumulate into larger truths. We must have theories, or explanations as they are called, of many of the facts which we observe. We need them to help us to the best ways available for arranging the facts, as we need bookshelves for books; and they are useful for suggestions as to what, and where, and how to search for new facts. But here, or hereabouts, should be the limits of their use; and the using of theories for purposes beyond these is full of danger, and often a great hindrance to progress.

As one looks back on the relations between the collection of facts in practice and the explaining of them in accordance with the theories which were in successive times accepted, and which were commonly called "general principles," and regarded as safe guides in the treatment of disease, we may fairly doubt whether the gain or the loss were the greater. You will see that in each successive generation every fact observed in practice was accepted, not merely as a fact, but rather as an illustration of some pathological doctrine of the time. Mercury was, at different times, an antiphlogistic, a depurative, an alterative, and I know not what besides; harmless words if they had not been used as facts; but because it was thought, not known, to be all these and more, it was so misused and did so much harm that it nearly lost all its just repute, and many disused it. Similarly, bleeding was practised in excess, not so much because of the good it sometimes did, as because the physiology of the time seemed to prove that it must be useful in a large number of conditions in which it was not. And because this was deemed sure, men overlooked the evidence that it was often useless and sometimes mischievous; they did not observe; they preferred thinking, and they thought erroneously, and were guided by their thoughts more than by the facts. Now, these were errors of men as honest and as clear-headed as any in our own day, and among them were some to whom medicine owes great help to progress. Surely, it should make us very cautious. I have cited old historic errors, but exactly similar modes of thinking are everywhere prevalent, exactly similar fallacies in letting doctrines rather than facts serve as our guides in practice. And we need the more caution because this assumed power of explanation is so much more attractive than the gathering of facts. The explanations are so much more our own; like very parts of ourselves; so much more impressive; they sound so learned; patients admire them; some, I have often thought, would rather have their maladies explained than have them cured.

I feel, again, that it may seem as if I thought of you all as entering on a course of study in which there would be only the misery of doubts and failures. It will not be all this. With due caution there may be great happiness as well as great utility in a life full of scientific observation. But it is impossible not to feel in one's-self and to see in others how great is the risk of error, how great the risk of utter waste of time in anything but careful study; not to see how an error once begun becomes much more dangerous when we put thoughts into words; and then the words, as if with right, assume the force of facts, and then become guides to ourselves and others, even when we have the care of human lives entrusted to us. And, too surely, the further men pursuing scientific knowledge go from facts, the wider and the pleasanter become the ways of error to those who do not watch and guard every step they take. It is not in the religious life alone that the allegory of the ways is true; equally in science, "strait is the gate, and narrow the way that leadeth unto" truth, "and few there be that find it." In both alike he that will may find the way; and in both alike he may do well to remember a saying of Faraday, who lived with equal earnestness both the religious and the scientific life. Faraday said: "The education which I advocate has for its first and last steps humility."

I said just now "he that will"; I have spoken of the methods of scientific study, of its machinery; but, it may be asked, what is to be the force? what the driving power? It must be each man's will, by whatever motive stirred; when the will is wanting the most perfect scheme is useless; and let me take a privilege of age, and say to the much younger who are here as students that this will must be used and cultivated now, and from first to last, and that the will is even much more important at first than at last; for at last will becomes habit, as certainly for mental as it does for muscular uses. You know this well enough, for every exercise of the limbs, for every kind of athletic dexterity; the movements which required at first the constant force and direction of your will become at last so easy that you are scarcely conscious that the will is engaged in them. It is nearly the same with mental work; but for this, equally or even more, the will must be rightly and earnestly exercised during the whole of student-life. Such as the student is, such will be the practitioner.

I have put in print an anecdote of Mr. Abernethy which bears on this; and let me say he ought to be remembered not as a rough wit or a mere eccentric, but as a man of rare sagacity, an admirable surgeon and teacher, and one of the most graceful scientific writers of his time. Entering his lecture-room at the beginning of a session, he looked round at the crowd of students and said solemnly, "Good God! what will become of you all?" Many years afterwards I tried to find an answer to such a question; tried in the dullest way—by statistics. I traced the careers of a thousand of my pupils. I need not give the numerical results; but the general and certain rule was as I have said: such as the students had been, such was the practitioner. There were some few exceptions, but the general rule was clear; and so you may be sure it always will be. So, if you are to be hard-working scientific practitioners, as I hope you will be, you must be hard-working scientific students; you must be always learning for yourselves; always observing, recording, collecting truths; searching for new truths as well as accepting those which you will be told by your teachers. And let me say again and again this will be the best way by which you may become as good practitioners as you possibly can be, and may go on in practice constantly becoming better and constantly acquiring fresh knowledge for others as well as for yourselves.

Often, when I have been urging these things, I have been told that in ordinary practice there is no time for observing or recording anything useful for science; and that, even if there were time for it, the cases are such as cannot be usefully studied.

Well, as to time, I hope you may all be overwhelmed with practice; and then you will be able to illustrate what I believe is usually true, that none find so much time to spare as they that have too much to do and do it with all their might. And as for the cases, I must admit that a very large number of the cases in private practice cannot be studied minutely or fully enough to yield useful knowledge; and certainly the practitioner may often look with envy on the physiologist, who can choose his study wherever novelties are most nearly within reach, and the renown of discovery may most easily be gained. Still, if there be only a mind for the work, any sort of practice will supply materials; science is possible anywhere and everywhere, even in small country-practices. There are evidences enough of this around us; or, if great examples are needed, Jenner was in such a practice when he discovered vaccination, and it was no chance-discovery, but the outcome of very careful analytic observations. Koch was a country practitioner in Silesia, when he first showed the plant-nature of a specific morbid poison. Mayer was a poor country-practitioner when he began his marvellous work on the correlation of forces; and nine-tenths of the honour due for the discovery of anæsthetics must be assigned to ordinary dentists, who worked without knowing it after a sort of scientific fashion. But you need not be ambitious for discoveries equal to those. Good will be done both to yourselves and to others by the observation and careful record of single rare cases or groups of cases of almost any kind, for, literally, nothing that is published is now lost.

I need not speak of the radiation of knowledge from scientific centres, such as you have here; it is diffused as far every year in all such meetings as those of the British Medical Association and of its numerous branches; further still, it is carried on and on by our medical journals; and from these, those admirable helpers of all good progress, the index-makers, with my friend Dr. Billings for their chief, and with Dr. Neale among ourselves, bring everything within orderly arrangement for those who will wisely search books as well as nature.

But, indeed, there are much larger subjects for which general practice, such as, I suppose, most of you will be engaged in, offers by far the best opportunities for study. Take, for instance, that of the inheritance of disease, than which there is scarcely one more important, either in daily practice or in the profoundest questions of pathology. Facts are wanted for its illustration, which none can supply so well as those who are distinctly family-practitioners, and can learn the histories of families. Hospital-practice is nearly useless for the study of the inheritance of the most important diseases, and consultation-practice is, as I know, scarcely better. If one ventures, in a serious consultation, to ask the relatives of a patient anything about consumption, scrofula, cancer, or even gout, the commonest answer is at once, "Oh, nothing of the kind has ever happened in our family," and the answer is often given as if one had suggested suspicions about the family respectability, as if one had asked whether the father of the family was really an honest man. The facts are either not known, or they are willingly forgotten; and, in any case, negative statements of this kind are nearly valueless. Most persons know what their parents died of or suffered with, and some will tell; but very few can tell much of all their grandparents, and I have only found two who could tell me anything useful about the illness or deaths of more than two of their six or eight great-grand-parents. Yet from any of these, to say nothing of still more distant ancestors, a tendency to any disease may have been inherited, and may determine the characters of any illness in the inheritor. It is common to see in a person still living a likeness in form or face to the portrait of some remote ancestor; and not rarely it is a likeness which seemed to have been lost in the intermediate generations. But likeness in external features is not more transmissible than are likenesses—whether in shape or other properties —in any other part; as in liver, stomach, heart, blood, or lungs. We can see the one likeness more easily than the others; but there is no reason or probability that one manner of inheritance should be more frequent or exact than another.

I can see no better hope of gaining facts on these points than by the knowledge which may be gradually acquired by practitioners who, by their personal knowledge, or by record or careful inquiry, can tell what have been the prevalent diseases of members of the same family, or of families intermarrying, during two or three or more generations. Doubtless the knowledge thus gained will be incomplete, but it will be better than any we yet have or are likely to gain by any other means; and its importance in pathology cannot be overstated.

I said just now that negative evidence as to the inheritance of disease is useless; but I should have added that this means only the evidence of patients and their friends. The negative evidence of family practitioners might be very useful; it would be so, for example, if it could tell what proportion of children and of grandchildren do really escape a disease which can be transmitted. Suppose a woman dying with cancer. How many of her children living, say, to sixty will have it? and how many of her grandchildren? I think I can be nearly sure that on the whole very few would; but no one really knows. It might be ascertained by fathers and sons practising together in places where families are apt to hold firm to their homes.

So, too, observations carefully made in private practice might clear up some questions concerning the mingling of diseases. Suppose, for example, that anyone were asked what are the probabilities of life and health in the members of a family of whose four grandparents one had gout, another cancer, another tuberculosis, another chronic rheumatic arthritis? I know no one who could answer. Yet cases of the kind are not rare and the questions involved in them are very important for practice, for pathology, for life-insurance. Every person may be reckoned as one on whom converge liabilities to the inheritance of any or all of the heritable diseases from which his ancestors have suffered. Now, do any of them in their transmissions and convergence modify and annihilate one another? Are any mutually exclusive? Unless some such change ensues, it is hard to imagine how our population should increase and maintain its working power. Or, again, are any capable of combining as in an organic compound? and what is that compound? Surely here are questions enough for anyone who in the opportunities of family-practice will study them carefully and record them. They are among the many questions that can be settled only by facts, and many of you will have the facts within your reach.

I could easily add to the list of subjects, for which the ordinary practice of our profession, whether in families or in hospitals, or dispensaries, whether private or in the public service, offers the best possible opportunities for scientific study. Such are the utilities and adaptations of proposed new medicines; the testing of them with much more than usual doubt and patience; the distribution of disease in reference to soil and other local conditions, and many more. Only learn to study after the right manner, and, as all who have had experience in it will tell you, you will find it a source of very rare happiness and utility, and of no harm whatever. There are some, indeed, who would tell you that the scientific man is ill-fitted for anything but science; that he cannot be punctual, business-like, a plain speaker, pious, or I know not what else. It would be difficult to find greater nonsense in any of the books or journals on a modern book-stall. There is nothing that a man may not be at the same time that he is scientific. I would not make light of anything that would hinder you from being business-like; for I should have to admit that I have known more failures in our profession through want of this quality than from the want of any other; more than from the utterest want of scientific or even of good practical knowledge. But the failures were not only among the scientific. Surely I need not say in Manchester that good men of science may be also good men of business; and what may be seen here may be seen everywhere. If a man of science cannot be business-like, it is the fault of his brain, not of his study; he would have been the same in any other pursuit in life.

Devotion to science may be combined with whatever is good or evil; but in our profession its associations with what is good are far predominant, and it may give a good direction to many mental dispositions which are apt to drift into mischief, or to folly, or to waste of time. But I need not dwell on these. It will be enough if you will bear in mind its utility for those whose lives and happiness will be committed to your care. As long as you live, the more and the better you study, the more you will know, and the more good you will be able to do; and this good will be of the kind that may be most real—the good not only of lengthening men's lives, but of increasing their working power and their happiness. And this is the better. Time is not the best or only measure of the value of life; for now, as distance may sometimes be best estimated, not by space, but by the time required to traverse it, so each life, as to its quantity, should be measured, not by years, but by the work done in it. The good that medicine and surgery may do is not half estimated by bills of mortality that prove only an average greater length of life. More good is done by making a larger part of each life more fit for working; and in this may be a measure of utility such as may be unmatched in any other calling. Herein may be your utility; herein will be your duty; herein your grave responsibility.

And one word more. We boast of the progress of medicine and surgery in the last fifty years. We try to tell all the good that it has brought to men. In the next fifty, through which I hope you will all live, the gain will be still greater; for the progress of knowledge is with increasing velocity. Now, how much will each of you contribute to this progress? None can tell. But this at least is certain, that you are bound, by plain duty, to do your best to contribute to it largely: even as it is every man's duty to do all the good he can by all the means that can fairly come within his power. And remember always that wherever power is, there is duty; and where-ever duty, there responsibility to God and man.

T. Sowler & Co., Printers, Cannon Street, Manchester.